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ABSTRACT

The underlying hypothesis of this paper is that the efficiency and effectiveness of educational organizations are affected by their structure. Primary emphasis of the paper is on investigating the link between organizational structure and organizational outputs in an effort to understand why different educational organizations are structured differently with respect to goals, decision-making, and so forth. Because the paper is intended to stimulate future research by organizational theorists, it is organized into seven sections that correspond to topical subareas within organizational theory. These sections focus in turn on the following topics: organizational goals; participation in decisions, decentralization and community control; administrative intensity; environment, structure, and strategies of control; interorganizational analysis; and methodologies of administrative experiments. (JG)

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ADMINISTRATIVE STRUCTURE,
EFFECTIVENESS, AND EFFICIENCY

A Prospectus For Research in
Organizational Aspects of Education

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ADMINISTRATIVE STRUCTURE, EFFECTIVENESS AND EFFICIENCY

INTRODUCTION

The hypothesis that underlies this report is that the outputs of education organizations -- their efficiency and effectiveness and, in general, the ways in which they respond to stimuli -- are affected by structure. The manner in which the institution is organized for work and the form of its administrative apparatus are presumed to affect what gets done, how much, and how well. It is almost unthinkable that this not be the case. It would be most surprising indeed to find that outputs were only minimally dependent upon strategies of control in the organization, the extent to which decision making is shared rather than centralized, the kinds of objectives that have de facto importance, and the relative size of the system's bureaucracy. It is true, however, that we are not positive that this relationship exists in every case, nor do we know the important details (i.e., which particular outcomes are affected by which particular dimensions and subdimensions of structure), nor do we know how strong many of these relationships are -- how important. Therefore, much of the organizational research to be described and encouraged in the following pages will deal precisely with exploration of this basic hypothesis -- the link between structure and output.

In organization theory, "structure" is interpreted and examined in several ways, and these will be reflected in the sections and subsections to follow. We consider the goal structure of the organization -- the kinds of goals, if any, that are widely shared in the organization and the kinds that dominate behavior, whether shared or not. We consider what might very

loosely be called the "power structure," that is, who helps make what kinds of decisions and whether control is exercised personally or through systems of formal rules. We also consider structure in the sense of the numbers of administrative and staff personnel relative to the complement of production personnel, such as teachers. In addition, we treat the important issue of the decentralization of education management in metropolitan areas to homogeneous subgroups of the population.

In all of these cases, we emphasize the priority of investigating the link between structure and outcomes. Although organization theorists may be curious to learn all sorts of things about structure, and although what is contemplated here is a program of basic research, still, for the purpose at hand it would not do to generate a large proportion of results on the causes and descriptive niceties of structure and only a small proportion on effects. On the contrary, the structural dimensions we should try to understand are the ones that seem to make a difference. We will see, however, that in many cases the largest part of the job of investigating either causes or effects comes in describing a structure itself, and research is needed for this task before the others can be satisfactorily completed.

Given that some aspects of structure do affect organizational outputs, it is important to understand why some education organizations are structured one way and some another with respect to goals, decision making, and so forth. For answers to these kinds of questions we largely look outside of the organization in question. We consider many characteristics of what is loosely called its "environment," and we also consider it more specifically

as a member of a multi-organization group with which it must interact and which inexorably affects its behavior. In other words, we do not operate on the premise that major organization structures are established by fiat and may be changed in the same way, but rather that education organizations are embedded in larger social systems and that their structural features have evolved largely in response to the characteristics and demands of the outside world. Lastly, it should be noted that at this point we seek to understand structure; whether we can do anything about it is a question that can only be answered once some understanding has been achieved.

The reader will find that the balance of this document is not packaged as one might expect from the above overview. It is not organized from the special perspective of an interest in improving education. Rather, it is packaged according to subareas and "invisible colleges" within organization theory. It is, after all, organization theorists who for the most part will do the work that is suggested, and it was felt advantageous to offer the task in terms of streams of intellectual history to which these researchers may readily relate. Thus, one finds sections headed: Organizational Goals; Participation in Decisions; Decentralization and Community Control; Administrative Intensity; Environment, Structure, and Strategies of Control; Interorganizational Analysis; and The Methodology of Administrative Experiments.

It will be seen that only the last of these, Administrative Experiments, is an area in which there is an advanced body of work upon which to build, in which the major tasks are now perfecting, refining, and specializing to education. Decentralization, Environment-Structure, and Administrative Intensity are areas in which there is only a moderately advanced foundation

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or else the combination of much usable prior research in some aspects but little or none in others. In these three categories, although carrying certain existing directions forward is important, there is also much new ground to be broken within the context and purposes of the broader research program visualized here. In the remaining three areas, even though some represent research enterprises that have been in operation for a good many years, it appears vital either to lay fresh conceptual groundwork or to be guided by conceptualization that has only recently been articulated. Following older paths would be to perpetuate inefficient motion. In every case, however, even though new departures are now necessary, a perspective that includes past work makes it clear that future work will with high probability be cumulative and productive. Otherwise, research areas were not included in the prospectus at all.

The order in which the seven topics are presented has only minor significance. The first four and in some measure the fifth represent ways of characterizing structure and therefore present research opportunities for exploring the link between structure and resulting behavior. The fifth and sixth focus primarily on determinants of structure, although to some extent these same determinants are viewed as direct causes of outputs, such as efficiency or morale. The last is a methodological topic that cuts across all of the previous ones. Although priorities among the sections would be extremely difficult to establish, the prospectus as a whole reviews a large amount of desirable research. Therefore, priorities and relative importance are suggested within topics whenever such distinctions are possible.

ORGANIZATIONAL GOALS

In organization theory, the dominant recent trend is to consider organizations as collectivities whose purposes lack an aggregate unity. The epitome of scholarly work in this tradition is the "garbage can model" of organizational choice suggested in a most stimulating recent article by Cohen, March, and Olsen (March 1972). In the specific kind of organization depicted by this model, problems, solutions, choice situations and decision makers move about from one structural locus to another, encountering one another almost irrationally, i.e., according to rules that have essentially nothing to do with organizational aims.

This trend is in stark contradistinction to a venerable tradition in social science and classical management that simply took for granted the basically purposeful nature of organizational behavior. The intellectual problems with this earlier tradition are that terms like "goal" were never seriously defined, obvious multiplicities of purpose went unrecognized, and the importance of self-seeking behavior in organizations was blithely ignored. The older tradition was therefore justifiably vulnerable to scholarly criticism; however, a model of purposelessness is as difficult to accept for all organizations as the equally extreme model is replaced. No doubt, some organizations have goals, some do not, and others occupy what is in some sense an intermediate position.

Whether or not organizations have goals is probably important from several perspectives, but we are not sure how important because we have rarely looked to see. Thus, we are faced with several questions of potential significance for education organizations, such as, "Are such

organizations indeed aimless? to what extent? under what conditions?
with what effect?

The last question is surely crucial, in the sense that if it makes no difference whether organizations have goals, or what kinds they have, then it is difficult to justify research to describe the goal structures and discover their causes. On the other hand, it is impossible to know what the impacts of various goal structures might be until we have first measured and described the structures themselves, and this work has barely begun. Furthermore, that descriptive task is no doubt the most difficult part of a research undertaking directed at understanding goals and their relationships with other factors. Let us guess on the basis of intuitive plausibility, however, that the intent of organizational members, as characterized in a number of different ways, does with respectable probability have something significant to do with what goes on in organizations.

On this basis, the pivotal research hypothesis takes the general form, Dimension X of the goal structure of organizations affects dimension Y of organizational behavior or output. In order to investigate this family of questions, it is necessary to conceptualize organizational goals in such a way as to yield a coherent set of interesting dimensions and to work out ways of measuring such dimensions of goal structure in education organizations. These steps then make possible a full and rich description of the goal structures of various types of education organizations (school districts, high schools; teachers unions, etc.) and of the variation in goal structure among organizations within any given type. Then, the hypotheses relating to outcome measures may be explored. And finally, if it is found that goal

structures do indeed make a difference in behavior and outcomes, hypotheses regarding the determinants of variation in goal structure can be formulated and investigated.

Types of goals

The first step, conceptualizing and dimensionalizing organizational goal structures, has basically been taken by Perrow (1970, 133-174), Gross (1968; 1969), and Mohr (1973), although there is doubtless room for refinement and elaboration of this early and largely nonempirical work. Considering the efforts of these authors in combination, it is clear that organizational goals are multiple; it is unprofitable to think of an organization as having one single goal. Also, goals are empirical; they should be discovered, not imputed.

The work of these writers also suggests that two central dichotomies form a productive four-celled paradigm for the description of organizational goal structures. First, there are both consensual goals (i.e., ends upon which there is a substantial measure of explicit or tacit agreement among organizational or program members -- these are the true "organizational" goals, if any exist) and operative goals (i.e., ends upon which there may not be broad consensus, but which nevertheless guide the behavior of the organization in important ways). In addition, it would be fruitful to document the "goals for" the organization held by certain individuals, such as the chief executive, even if they are neither consensual goals nor operative goals. Second, goals may be either externally oriented (e.g., pupil achievement) or internally oriented (e.g., prestige of the district), sometimes

labeled transitive and reflexive, respectively. Each of these latter two types may be divided into several subtypes (see Gross, 1969). Within each category of the paradigm, it is also important to consider subgoals - objectives that are instrumental to the achievement of the larger purpose but are not properly ends in themselves.

Variations on this scheme for conceptualization of goal structures are certainly possible, as noted earlier, but the state of current thought suggests that we proceed on some basis similar to the above. The most prominent alternative is the idea of organizational goal as a large constraint set, as proposed by Simon (1964). There is a strong sense in which Simon's conceptualization appears "correct," but it is not intended as a foundation for research on goals; it is too inclusive and unorganized to serve as a conceptual base for the sort of research contemplated here.

Dimensions and measurement

An early requirement in this broad area of study is to compile lists of goals on which to base research in given types of organizations; it is not enough to ask open-ended questions, for not only might answers be inadequately comparable across organizations, but much of significance might be omitted entirely. Such compilations can be accomplished in part through library research and the consultation of experts, but to be done well the task merits exploratory research in representative organizations of the types to be investigated. Since the lists of goals will color all of the ensuing research enterprise, such empirical exploratory research is greatly to be encouraged.

For each goal of any of the above types, basic variables are created by characterizing the goal along certain dimensions. The dimensions that would seem most important from current literature are clarity and operationality (March and Simon, 1958), extent of consensus (Mohr, 1973), both actual and preferred importance (Gross, 1968) and location, that is, the organizational strata or units to which the goal is salient. When individual goals are aggregated into categories (e.g., "transitive"), then each dimension, as well as differences among them, leads to numerous research hypotheses, e.g., "The more important are transitive goals relative to reflexive, the lower will be the level of organizational conflict," or, "The smaller the program, the greater the degree of consensus on executive goals."

The literature contains little guidance on specific instrumentation, except that Gross (1968) provides a good example for the measurement of importance and preferred importance. The question of whom to ask about an organization's goals is discussed in part by Mohr (1973, 478), but many aspects of the issue are left for resolution in future empirical work.

Descriptive research

One by-product of the thorough conceptualization of an organizational goal structure is that it enables its description in any organization, or in any group of organizations of a specific type -- high schools, inner-city schools, Kentucky school districts, etc. Basically, we are interested in goal structures because they make a difference, or at least because we want to test whether they do indeed make a difference in particular behaviors or outcomes. But it may not be immediately apparent just what sort of a

difference to look for, just what kinds of relational hypotheses to formulate. To some extent, ideas will grow out of detailed knowledge of what the goal structures of different sorts of organizations actually are. Therefore, descriptive studies of the full goal structures of various types of educational organizations are needed, quite apart from hypothesis-testing research relating certain aspects of them to other variables. For clarity, the following are a few examples of the many forms that conclusions from such studies might take: "Large school districts have essentially no consensual goals." "In state departments of education, the goals of the chief executive tend to have little impact on organizational behavior." "In individual schools, subgoals take precedence over goals in importance and tend to displace the goals they are designed to attain." In universities, reflexive goals take precedence over transitive goals, even though the preference of organizational members is for the reverse" (see Mohr, 1973, 480).

In this regard, it will also be productive to compare the goal structures of education organizations to those of recreation departments, hospitals, welfare agencies, and public service organizations of other types. Comparative research of this sort will stimulate ideas on why the goals of school systems, say, tend to be what they are, and what might be done to alter the purposive orientations of individual teachers and administrators.

Effects of variation in goal structure

Once goal structures can be measured, their characteristics can be explored for relationships with possible consequences or effects. Some,

but not all, of the outcomes upon which goals have an intuitively plausible effect are: the morale of organizational members, their commitment to this particular organization, the intensity and distribution of conflict within the program or organization, the dominant mode of decision making (e.g., a rational, problem-solving mode vs. a bargaining or political mode), the effectiveness of the organization in achieving certain of its own goals (or certain results, such as pupil achievement, whether intended or not), and organizational flexibility or innovativeness. For illustration, one hypothesis might be, "The more operative are (certain subtypes of) reflexive goals, the lower will be the morale of organizational members." Although priority should perhaps be given to school districts and schools as organizations, there is also interest in exploring the effects of goal-structure variation upon the above outcomes in universities, teachers' unions, school boards, state agencies, certain subdivisions of many of these, and other education organizations.

Goals may have effects on other variables frequently studied in organizational research, such as decentralization, formalization and other aspects of bureaucratic structure. Further, these latter may be thought of as instrumental to the achievement of outcomes such as those reviewed above. Before devoting resources to such studies, however, one should have good evidence that the intervening factors are indeed closely related to outcomes in which we have quite an independent interest. On the other hand, characteristics of goal structure can well be investigated along with other factors as part of more complex models explaining certain outcomes. Goal-structure characteristics may be related to participativeness or decentralization, for example, and morale in the organization may well be dependent upon both.

Causes of variation in goal structure

Lastly, given that goals do have a significant impact on outcomes of interest in education, research is in order to determine why goal structures differ in certain ways from one organization to another. We know so little about the differences that do exist among organizational goal structures that it is difficult to speculate on their causes; descriptive studies, especially comparative ones, will be most helpful in this regard, as suggested earlier. Moreover, not only are studies that compare different types of organizations important for hypothesis generation, but so are comparisons of goal structures of the same organizations across time. Organizational goals are unquestionably dynamic. In broad outline, however, they may well not be susceptible to fluctuations due to short term forces or minor exogenous shocks. Studies covering even two points in time may well be effective in suggesting the kinds of forces and conditions that intervene to cause major changes in purposive orientation -- long-term changes in the availability of resources, shifts from internal growth to decline, changes in average tenure of personnel, changes from one stable leadership to another or from unstable to stable, etc. Other determinants that have been or might well be explored initially are the stability and certainty of the organization's environment (see Duncan, 1972), organizational size, age and growth rate, type of ownership (e.g., public vs. private universities -- see Gross, 1968), leadership philosophy, and political structure of the jurisdiction -- especially the degree of party competition.

As a final note, it should be recognized that goal structures are continuing rather than discretely occurring phenomena and may therefore be

the effects of some factors assumed to be consequent, and vice versa. The causal ordering of goals relative to variables such as leadership philosophy and organizational effectiveness is problematic and a matter of substantial interest. The design of research can and should take such ambiguity or reciprocity into account.

II. PARTICIPATION IN DECISIONS

Much scholarly work has been carried out with reference to what is called "human relations" in organizations. Some is designed to further our understanding of small-group processes and their impact on organizational function. Other research has focused on the quality of certain relationships, particularly on the supportiveness of supervisory behavior. Still other research has investigated the extent to which subordinates (at any level) either make decisions on their own or at least help make the supervisory decisions that are relevant to them. We concentrate here on the last of these streams, primarily because it is a particularly important dimension of the structure of an organization. Furthermore, there has been an unfortunate tendency, especially in field experiments in organizational change (i.e., changing the "human relations" in an organization), to confound the possible effects of these three types of independent variables by altering them all at once; it will be a healthy development to keep them distinct in research so that their respective "main-effect" contributions can be sorted out.

Research on participation is largely rationalized by the hypothesis that there is a positive relationship between that dimension and important outcomes with which this prospectus is concerned -- effectiveness, efficiency,

and, by extension, morale (job satisfaction), commitment to the particular job and organization, innovativeness, and perhaps others. We will briefly review the trend of findings bearing on this general hypothesis, discuss some of the reasons for the trend, and outline the kinds of research steps it therefore seems appropriate to take in the future.

Inconclusiveness of findings. Morale and job satisfaction have quite consistently been found to be related to participation, often quite strongly (Likert, 1961; White and Ruh, 1973). If it were believed that job satisfaction in education is a determinant of effectiveness, efficiency, etc., or if satisfaction were simply valued as an end in itself, then these findings would appear to suggest that we attempt to introduce as much participativeness as possible into education organizations. We will point out below, however, that these findings are ill understood and could be misleading.

The idea that a high level of commitment to job and organization results from participation in decisions that are relevant to one has been frequently advanced (Likert, 1961; Etzioni, 1965, 693-695; Rossel, 1970). In light of such attention, it is surprising that the variable has appeared so infrequently in empirical research testing the connection, and is not very explicitly measured when it does appear (see, e.g., White and Ruh, 1973, 508-509).

What evidence there is indicates that there is a moderate relationship between participation and commitment, but, given the measurements, the evidence is not satisfying and the apparent relationship is not as strong as one would expect.

The results relating participation to innovativeness are also inconclusive (see Zaltman, 1973, 143-146). Mohr (1971) found a small relationship. Hage and Aiken (1967) found a fairly strong relationship using one measure of participation and a very weak one using another.

Research findings on the relationship between participation and effectiveness (e.g., productivity) are perhaps the most unsatisfying of all. The association appears to vary from weak to moderate in some kinds of jobs and to be nonexistent in others (Likert, 1961, 26-60, 89-96).

There is also the hypothesis that whether or not participation has these positive outcomes depends on how much the employees concerned value participation -- an interaction hypothesis (in the statistical sense). After reviewing conflicting findings on this proposition and then not finding in their own data the strong statistical interaction that they expected, White and Ruh find themselves unable to reach any reasonable, definite conclusion (1973, 512-513).

Problems of theoretical formulation. To a great extent, the view that research findings in this area are puzzling and unexpected results from a failure to specify carefully just what is to be expected and why. In the case of job satisfaction, for example, there is little reason to expect all organization members to react in the same way to participation; as the interaction hypothesis stipulates, those who value participation should react to it much differently in terms of satisfaction than those who do not. Social scientists who accept the high correlations that have appeared as support for the hypothesis are accepting a hypothesis for which there would appear to be little theoretical justification. It would make more

sense, in fact, for the investigators to question the surprising strength of the correlations (as do White and Ruh, 1973).

In the case of commitment, difficulty stems from a failure to specify very precisely what is meant by the term, that is, to offer a conceptual definition. We should expect a small impact, perhaps, of participation "in general" upon a general feeling of commitment to the job. But the primary logic of the participation-commitment hypothesis specifies that the act of participating in a decision -- being a part of it -- will have the effect of causing one to work conscientiously to carry it out. This logic should lead one to test whether participation in specific decisions affects commitment to the specific goals or procedures that were decided upon. Such tests are at best rare in organizational research.

Similarly, one might seek to specify why participation is hypothesized to affect productivity or other indicators of effectiveness. To the extent that participation and effectiveness are measured "in general," it would appear that morale is believed to be the operant intervening mechanism, i.e., high morale results from participation and good, conscientious work results from high morale. It is well known, however, that research has failed to demonstrate the latter of these two links (Kahn, 1960). It is much more reasonable to expect commitment to be the intervening mechanism -- when individuals help make a decision, they feel committed to carrying it out, and their resulting effort leads to effective implementation. As with commitment, however, this logic would lead to a measurement of effectiveness in specific jobs, programs, tasks, or innovations -- (the ones for which

there was measured participation in the basic decisions -- rather than productivity in general. One should not on this basis, for example, expect participation in administrative or caretaking decisions to lead to a high quality and quantity of line productivity.

The case of innovativeness is more complex. There is reason to expect that much supportive group process, or even the frequent consultation of an individual for his or her opinions, will actually evoke innovative ideas and will also permit the open treatment of reasons for resistance to change. On this basis, the more participation there is, the more innovativeness we should expect to find in general. Thinking of the acceptance by employees (e.g., teachers) of innovations introduced or suggested by management, however, as in the classic studies (Coch and French, 1948), the situation becomes much the same theoretically as the case of effectiveness. Whether or not participating employees accept a proposed innovation will depend on how they see its affecting them (assuming no coercion or manipulation); if they do accept it, however, the participation should increase the chances of successful implementation, through the intervening mechanism of commitment.

In sum, theory connecting participation with valued outcomes is not simple. Future research should reflect this by utilizing designs and measurement that accurately represent well-considered theoretical underpinnings, rather than exploring oversimplified bivariate relationships.

Problems of measurement. The puzzling state of research findings in this area is due partly to the measurement of concepts, as well as to the theoretical problems just reviewed. At the core of much of the difficulty lies the measurement of participation, itself. Vroom (1960, 10)

found strong evidence fifteen years ago that objective participation and psychological participation can be radically different. We should not therefore expect the amount of participation people say they experience (a lot, a little, etc.) to be strongly related to the amount they actually do experience. This fundamental result has not been adequately utilized in past research, but it must be accorded proper attention in the future. Research results will not make sense as long as these two quite different operations come indiscriminantly under the same conceptual heading, -- "participation."

There is good reason to believe that extent of participation as reported by employees is not a faithful representation of events, but is rather a difference between what actually occurred and what was expected, or desired (i.e., the value placed on participation by the respondents -- see Mohr, 1975, 25-46; Crepine, et al., 1975, 65). A moderate amount of actual participation, for example, would be reported as "a lot" by those who expected very little and as "a little" by those who expect a lot. We therefore have interest in three operational quantities, and would use them for quite different purposes: the amount of participation people feel they should get, the amount they do get, and the difference between the two.

The measurement of other concepts also needs attention, particularly commitment, effectiveness, and the implementation of innovations. The primary difficulty is that they tend almost invariably to be measured quite broadly, as, for example, commitment to the job or organization, or overall record of effectiveness, whereas theoretical concerns demand that they frequently be measured with reference to specific decisions, tasks, programs.

or innovations. The latter kind of measurement is perhaps more costly, but the former has not proved to be an efficacious substitute in past research and therefore should be deemphasized in the future in favor of new departures.

Mapping participation as a value. It appears virtually certain that expectations with regard to participation vary widely and in interesting ways among individuals, and that this variation has an impact upon the results that participation, or the absence of it, will achieve. (Much of the research relevant to this conclusion is reviewed in White and Ruh, 1973, and Mohr, 1975. For a study specific to teachers, see Alutto and Belasco, 1972.) The time is now ripe to begin mapping the variation in expectations and values in various occupations and to learn, at least to some extent, what forces might be responsible for it. Such research is needed for two reasons. One is that participation must for some purposes be measured relative to the desire for it, as discussed above; we therefore need to know quite a bit more than we do at present what to expect to find in the way of variation in desire and how best to measure it. Secondly, as was the case with descriptions of goal structure, we will no doubt know much better what to hypothesize about the effects of participation when we have good descriptions of how the desire to participate actually does vary among employees, and why. Descriptive research of this type will, for example, enable us to determine to what extent and in which broad areas the desire of administrators for autonomy in decision making conflict with the desires of lower-echelon personnel for influence.

The measurement of desire for participation, especially for exploratory, descriptive purposes, should not be simple. The important subdimensions include (a) the specific areas, and their number in which some degree of authority is desired or felt to be proper, (b) the degree of authority or participation desired (e.g., the right to be heard, to have a vote, to veto), (c) the intensity with which this degree of authority is desired, and (d) the extent of agreement within a work group or peer group on the importance of such participation.

The effects of participation. Many research projects have been carried out in this venerable area, but it now appears that the job must essentially be done over again, with closer guidance by theory as it has developed, and measures more pointedly designed in terms of the new theoretical directions. The above discussion suggests the outlines of such research.

In the case of morale and job satisfaction, it is now clear that what must be carefully reviewed or conducted are studies in which the measure of participation is objective. It is highly problematic and will be most interesting to learn whether much of a relationship exists between actual (objective) participation and satisfaction (or morale). Most (but certainly not all) previous studies, however, have used a measure of reported participation, which is essentially what Vroom (1960) called "psychological" participation. We see now that asking, "How much do you participate?" is much the same as asking, "How satisfied are you with your level of participation?" It is small wonder that correlations of such measures with job satisfaction have been uniformly high. The high correlations tell us little, however, and there is little reason to continue to collect them.

In attempting to study the relationship between participation and commitment (and therefore, indirectly, effectiveness), the measurement of participation becomes more complex and also more informative. There is good reason to try several methods, the important guideline being that degree of commitment (or lack thereof, ranging to absolute sabotage of organizational effort, perhaps) depends not only on degree of participation, but also on the extent to which participation in that area was desired. One might, to take just one illustrative example, begin with a scale of actual participation ranging from -3 (for no participation) to +3 (for a great deal) and also a scale of desired participation ranging, say, from 0 to 1. The participation scale used to predict commitment would then be obtained by multiplying these two together. Furthermore, the measure of commitment would be tied in each instance to a specific program, policy, etc., concerning which participation might or might not have been permitted.

The multiplicative scale of participation just described raises the question of interaction noted earlier. As articulated in the literature, the interaction hypothesis must be interpreted as calling for the measurement of objective participation and predicting that it would affect satisfaction and commitment strongly when certain attitudes were favorable toward participation, but weakly or not at all when they were not favorable. Using "desired participation" as a summary for these attitudes, the illustrative, multiplicative scale just offered provides a predictive variable that would test essentially that same interaction hypothesis. If reported participation is used instead of the objective measure, however, it is plain that the relationship would not be expected to vary in subgroups as predicted by the interaction hypothesis,

but would be expected to be approximately the same no matter how favorable the attitudes toward participation. White and Ruh (1973) did use reported participation and did obtain these uniform results, which to them were puzzling in the extreme. Their study was an excellent one to undertake within the research stream to which they meant to relate and it should be repeated, but with objective participation measured instead of reported participation.

Recalling that puzzling and inconsistent results have been obtained not only by White and Ruh but by a great many investigators in the human relations area, a thorough review of findings is now indicated. It is possible that the inconsistencies may be largely explained by some combination of the interaction hypothesis and the importance of the measurement of objective vs. psychological participation. If not, then such a review is highly likely to suggest what factor in addition to this combination is needed to make sense, finally, of the large quantity of potentially important results now lying unused.

Causes of participation. Relative to the large body of research considering participation as a causal variable, very few studies have treated it as a dependent variable and sought to discover its own causes (exceptions are Lowin and Craig, 1968, Blankenship and Miles, 1968, Rossel, 1970, Vroom and Yetton, 1973, and Mohr, 1975). Assuming that the style of leadership in education organizations, taken as ranging from autocratic to democratic or participative, does have some impact on morale, efficiency, effectiveness, and other outcomes, the further question of the causes of variation in style seems most reasonable to address in research. This would be a departure from common practice in the human relations field, which has been to attempt

somehow to train people in organizations to be more democratic. The training has not been highly successful, all in all (Dunnette and Campbell, 1968, Argyris, 1968, Buchanan, 1969, Bowers, 1973), and from the applied point of view it bears supplementation. The newer departure would seek to learn the causes of variation in leadership style as it exists empirically in operating organizations, particularly schools and other education organizations. With such information, the levels of participation might be affected by attention to job content, information flows, and the fit between people, rather than only by attempting to train people to be more democratic, or just telling them to do so. Some of the factors that have already been found to have an apparent impact on leadership style are the degree of commitment to the job that upper echelons need from lower echelons, the degree of affect between supervisor and subordinates, the manager's perception of the decision-making potential of those he supervises, the concern of the manager to maintain status, and the hierarchical level of the organizational members involved. These forces can no doubt be combined to construct a more completely specified model of leadership style than any used to date. In addition, much has been discovered about measurement and interacting conditions in the few projects in this area that have been carried out, so that future research is now in a position to demonstrate substantial progress.

III. DECENTRALIZATION AND COMMUNITY CONTROL

For an issue that aroused such intensity of feeling -- hope on the one hand and fear and opposition on the other -- the life span of school decentralization as a subject of public debate was surprisingly short. The

Idea for a reform which would transfer some control over school personnel, programs, and finances to locally elected lay boards in submunicipal school districts developed principally in New York City between 1966 and 1969.

While the actual decentralization bill that passed the New York state legislature in 1969 did not fully meet the expectations of community control advocates, it represented nevertheless the first effort in modern times to restructure the governance of public education through the devolution of power outside the school bureaucracy. Detroit followed suit soon after New York, but no other city has joined the first two.

With the accomplishment of decentralization in New York, political activists, the press, and the public at large seemed to lose interest in decentralization. Nevertheless, proposals for school decentralization in a host of other cities had been set in motion and numerous studies had been commissioned by cities and states (see LaNoue and Smith, 1973). In other cities community-controlled schools, independent of the public school system, were being established (in Milwaukee and Washington, for example), seeking their funding from private, church, and university sources. One question which clearly emerges is: What happened to these various explorations and experiments?

Community control, or the political decentralization of municipal services, of which school decentralization was only the best known and most glamorous example, was once thought to be the answer to a whole variety of municipal ailments, especially those which afflicted the poor and powerless. Not only was community control of municipal services expected to make service planning and delivery more sensitive to localistic concerns, but advocates

hoped in addition that such reforms would provide psychic benefits (e.g. increased feelings of political efficacy) as well as the means around which to mobilize neighborhood political capabilities (Schmandt, 1972, 576).

While the eclipse of the issue of school decentralization in particular might appear to represent a loss of faith in the validity of that type of reform, it must be emphasized that the data on which to make such a pessimistic judgment simply do not exist. It is eminently reasonable to allocate resources to the sorts of efforts required to make an adequate evaluation.

Research in the area of decentralization in general has to date usually fallen into one of five different areas, which, taken together, represent a prologue to more systematic investigation. One body of literature consists essentially of arguments over the desirability of decentralization in the cities (Altshuler, 1970; Fein, 1971; Kristol, 1968). Fein's The Ecology of the Public Schools is perhaps the most comprehensive and lucid advocacy of school decentralization in particular. A second body of writing has been concerned with the construction of blueprints for community control. These are not only marked by the breadth of their scope but by their somewhat utopian quality (Kotler, 1969; Farr et al., 1972). A third category, which overlaps some of the others, has offered typologies and analytical frameworks for classifying decentralization experiments (Nordlinger and Hardy, 1972; Yates; Eisinger, 1971). A fourth category includes a number of case studies of the decentralization experiments ranging from simple descriptions of a whole range of relevant institutional reforms in several cities (Washnis, 1972), to detailed single-city, single-experiment examinations (Nordlinger,

1972; Fantini et al., 1970). Finally there have been several attempts of a comparative nature to examine the consequences and other correlates of community control experiments (Yates, 1973; Cole, 1974). None of these latter works addresses the issue of school decentralization in particular and none speaks to all of the critical issues that might be raised. The following broad topics represent appropriate areas for research.

An inventory of school decentralization programs

No up-to-date inventory of school decentralization experiments exists (cf. Education Research Service, 1969). Such an inventory effort should be undertaken to collect systematic information on the scope and durability of pilot programs, both public and privately funded; on municipal decentralization proposals under study and debate; and on state legislative efforts to enact enabling legislation for urban school decentralization. While some attempts to inventory other types of decentralization experiments have appeared recently (Zimmerman, 1972; Steinberg, 1972), the education field has not been the subject of recent surveys.

An investigation of the demand for decentralized schools

Several early investigations, conducted virtually at the peak of the debate over school decentralization, suggest that the mass demand for such reforms scarcely existed (Eisinger, 1973; Aberbach and Walker, 1972). The Eisinger study concluded that the demand, such as it was, was primarily an elite demand. Data indicating more general support emerged in a study of New York neighborhood leaders and residents conducted by Yates (1973).

More extensive examinations of the level and scope of the demand would be useful now. The importance of ascertaining the character and dimensions of mass opinion on school decentralization is that it will provide one gauge of the political feasibility of school decentralization as well as providing guidelines for the shape such reforms might take. It is possible, for example, that the demand is no longer associated so much with urban black communities as it is with lower middle and middle class white ethnic communities within the city, whose members see decentralization as a way of combating school integration. Indeed, the experience in New York City indicates that decentralization appears to have impeded integration, although systematic evidence for this effect is not available.

While the level of mass demand and interest in school decentralization may be viewed as a way of characterizing the supporting and shaping context in which the reform would be implemented, it may in the long run be more important to investigate the views on decentralization of local political elites (at both the neighborhood and municipal levels), of school professionals, of school board members, and among state legislators. Scattered evidence reveals opposition among school board members and administrative personnel in several cities (Fantini and Gittell, 1973, 46-47). How decentralization is perceived in state capitals, how the issue is and has been brought to the attention of state legislatures, and whether it is an issue in state education offices are thoroughly neglected problems.

The correlates of decentralization

What sorts of cities have been most receptive to decentralization efforts in all their variety? Is it a response to growing black political power?

Is it more likely to occur in cities in fiscal straits rather than in solvent ones? (If so, then we might interpret decentralization as an effort to shift the responsibility of failure from the city to the neighborhood. This in itself is a theme which merits exploration.) Is school decentralization more likely to become a subject of debate in cities with large populations of black school children? in cities with large and distinct white ethnic enclaves? in cities with weak or strong teachers' unions? in cities with low or high average reading scores? Where, in short, are school decentralization in particular and community control experiments in general most likely to surface as issues and where are they likely to be implemented?

Problems in the implementation and operation of decentralized school systems

While the case studies of the New York decentralized system provide us with a good sense of how the idea came to be translated into practice, no rigorous, analytic investigations exist concerning the dynamics of the operations of these school districts. (Nor do studies exist of Detroit or of the various experimental programs.)

Several issues seem worthy of attention. One involves the nature of conflict in the school districts. One aspect of this concern is the nature of the contest for control of the school boards. In both New York and Detroit, white voters dominated the low-turnout school board elections and thus elected local boards disproportionately dominated by whites. This occurred even in many districts which had black and Puerto Rican pupil majorities.

Are these white voters, apparently held together by ethnic and church ties, organizing to retain their power on the boards vis-a-vis blacks, and if so,

what strategies are they using? How are black organizations and groups dealing with the challenge of white power in districts in which blacks predominate? In many districts, both in New York and in some of the experimental districts funded privately around the country, conflicts on the boards occur among minority group members. What are these conflicts about? Are they related to ethnic or class cleavages? Of what consequence are they for school governance, and, indeed, for the broader political capabilities of low income groups?

Another aspect of conflict in the school districts involves the ancient problem of the clash between experts and laymen in a democratic system (Dahl, 1970, 28ff.). How does one allow "legitimate" exercise of competence or expertise and yet constrain it in such a way that expertise does not invariably become the sole criterion of decision? Democracy -- including the democratic governance of public education -- ideally requires that the expert serve the people. Such a notion has naturally been threatening to school professionals or education "experts." To what extent does such conflict exist in the various decentralized school districts? Over what in particular does it occur, and who wins? How do professionals adapt to the supervision of neighborhood lay boards in contrast to the centralized board supervision of before? No major studies exist of the professionals' adaptation to the decentralization of policy-making power.

A different sort of problem which arises from the implementation of school decentralization involves the impact of the reform on school financing. Does decentralization have any sort of redistributive effects? Does it cost more or less than a centralized system? What internal budget reallocations have been made necessary by decentralization? A question which will

require a long term perspective concerns the relative willingness of taxpayers to bear the costs of centralized versus decentralized school systems, measured, among other ways, by the behavior of school tax rates and the success of bond referenda. Studies have been done on the organizational and administrative costs of decentralization experiments in areas other than the school system (Yates, 1973), but the finances of school decentralization in both the public and privately funded types have not yet been examined.

Impact studies

Many decentralization studies scarcely last long enough to allow social scientists to determine impacts, but there are some which have attained sufficient longevity to permit such investigations. A few of these have lasted six or seven years, time enough even for longer term impacts to begin to make themselves felt. There are three major types of impacts for students of school decentralization to seek to measure and observe: effects on learning and performance in the academic realm; impacts on school behavior and attitudes not related to purely academic objectives; and political impacts on the adult participants and population in decentralized districts. Only the latter has been the subject of much research, although little of that research has taken place in decentralized school districts. Important variables to take into account in the assessment of impacts would include the class and racial composition of the districts and the school boards, the source of funding for the program (the independent schools should be compared with the publicly funded districts), the existence and use of paraprofessional personnel in the classrooms, and, of course, the nature of the power of the school boards.

Academic impacts are not likely to emerge clearly in the short run. When changes do occur, it will be difficult to sort out the effects of decentralization from other factors. For example, reading scores in New York have recently shown a slight upward turn after a period of steady decline. But whether this can be attributed in any degree to school decentralization or to one or more of a variety of other factors is thoroughly unclear at the moment. Certainly the relationship between decentralization of public education and academic performance is muddled by a host of intervening variables which are as yet unspecified. Dependent variables to measure academic performance include scores achieved on standardized tests, IQ tests, and graduation rates, among others.

Non-academic impacts might include truancy rates (attendance rates are dropping in New York), the rate of vandalism against school property (do people vandalize their "own" schools as much as those run by the central school board?), and the level and nature of discipline problems.

While the question of whether decentralization would improve services has scarcely been addressed (curious, since this was a major rationale for such reforms), researchers have devoted some energies to an examination of the political consequences of decentralization for the participants themselves. Cole (1974), Marshall (1971), Yin and Lucas (1973), and Yates (1973) have all been interested in the impact of participation on political efficacy levels in a variety of community control experiments, although none of those studied were school districts. While Yates cautions that political decentralization does not invariably lead to high efficacy scores, the other researchers find that efficacy generally rises in such situations among those

on elected boards. Yin and Lucas, however, in their review of extant survey data on mass level respondents find that while sense of efficacy seems to rise, trust in government is unaffected by decentralization.

Researchers must not only be interested in the political impacts of decentralization; attention should also be directed to the impacts of larger political developments on the demands, shape, and operation of decentralization experiments. For example, has the achievement of political success by blacks through the electoral system in some cities diminished energies that once went into the search for alternative ways of exercising power? What is happening in Detroit and Atlanta where black mayors govern? Are whites demanding community control in cities governed by blacks?

School decentralization reform is fraught with innumerable problems. Its political feasibility has always been doubtful and its educational validity has never been established. It seemed in the late 1960's, however, to be a powerful symbol for politically emergent minority communities, a fact which, in itself, made it important to take seriously. Indeed, one of the earliest arguments in favor of decentralization was essentially that cities should try such reforms because blacks wanted it: it was to be a demonstration of good faith (Altshuler, 1970). What the present status of the demand for decentralized schools is and what the impact of those existing programs has been are virtually unknown. Such reforms should neither proceed nor be abandoned until more is known about them.

IV. ADMINISTRATIVE INTENSITY .

Questions about the level of administrative intensity in organizations have been of considerable interest to social scientists. Much

cumulative work has already been carried out. In proposing areas of further study, it is useful to summarize briefly the contents and problems of this research, paying particular attention to its relevance for the organization of educational services. Our remarks will be organized around the following topics: problems of definition, problems of measurement, the determinants and consequences of administrative intensity; and alternative approaches to the study of administrative intensity.

Problems of definition

The issues which have motivated an interest in the administrative, staff or clerical component of organizations have been diverse in origin but similar in their implications. They include questions of the effects of size or economies of scale (Terrien and Mills, 1955; Haas, Hall and Johnson, 1963; Indik, 1964; Blau et al., 1966; Klatzky, 1970); the division of labor which creates a need for administrative and/or staff personnel to coordinate the functions of production personnel (Rushing, 1967); the growth of bureaucratization in organizations (Bendix, 1956); rising overhead costs (Melman, 1951); control losses across hierarchical levels (Pondy, 1969); growing structural or functional complexity of organizations (Anderson and Warkov, 1961; Hawley et al., 1965; Starbuck, 1965). The similarity occurs in the hypotheses, which in most cases predict that size as a scale factor will reduce the relative size of the administrative or staff component; whereas complexity, the need for coordination, bureaucratization, control losses etc., increase the administrative or staff component. It is relevant to note that each of these studies takes for granted the distinction between production personnel and administrative or staff personnel, although these components

have been variously defined in different studies. Furthermore, most studies have treated staff or administrative personnel as a homogeneous component, although at least one study has taken issue with this assumption (see Rushing, 1967).

Educational researchers have looked at similar issues from a somewhat different perspective. They have also been concerned with economies of scale -- a concern motivated primarily by considerations of efficiency in operation of school districts of different sizes (see Hirsch, 1960; Riew, 1966; Cohn, 1968; Osburn, 1970). However, they have generally treated as a dependent variable some measure of operating expenditures per pupil. Note that the single largest component of operating expenditures per pupil in school districts is salaries, and total expenditures on salaries in a given school district can be completely defined as follows:

$$\text{Total Salaries} = \sum_{i=1}^k \left[\frac{(\text{No. of Personnel in Category } i)}{(\text{No. of Pupils})} \right] \times (\text{Average Salary in Category } i) \times (\text{No. of Pupils})$$

where i refers to categories of personnel. Therefore, it is easy to see that the issues are related; operating expenditures per pupil is a highly aggregated variable which includes among its components the salaries of administrative and staff personnel and the number of these personnel.

Each of these approaches has its merits and defects. The literature on complex organizations has the advantage of looking at administrative personnel from a comparative perspective -- that is, researchers have, to some extent, been aware of the exigencies of different kinds of organizations which create needs for a larger or smaller administrative component. Future research on educational organizations could benefit from such a perspective if only because researchers would be better able to make explicit what

intrinsically distinguishes education as a service from the service or functions performed by other kinds of organizations and what is merely an artifact of the way education has traditionally been organized.

A principal drawback to the comparative approach adopted by organization theorists is that it often takes for granted that organizations are comparable and then defines variables so as to make them comparable. The administrative component is a good case in point. By dividing personnel into administrative personnel and production personnel, organization theorists have assumed: 1) that the distinction is meaningful, and 2) that it is unnecessary to consider what the "administrative" or "staff" personnel actually do, and that what these personnel do in different kinds of organizations may be completely different (cf. Freeman, 1973). Both of these assumptions are questionable if we consider educational organizations. For one thing, personnel cannot be neatly categorized into administrative and production personnel in school districts. Teachers spend considerable time performing intrinsically clerical functions; principals who might be considered administrators are often teachers as well; and while their time might be prorated between these functions, such a division would have to be somewhat arbitrary. The only purely administrative components would be superintendents and their staffs, and the size of this component is highly constrained by the role definition of superintendent (i.e., there is one to a school district). Publicly available data on school districts distinguish "administrators" from "instructional personnel"; but from the standpoint of organization theorists administrators, as defined in school district data, are only a small (and probably the least variable) of the administrative.

staff or clerical component. The fact that the purely administrative component is relatively fixed by the role definition of superintendent and the traditional organization of school districts also suggests that other categorizations of personnel would be of more use in studying educational organization (and other organizations as well) than the division into administrative vs. production personnel. For example, if any component is discretionary in school districts it is probably that of educational specialists (e.g., psychologists, social workers, psychometrists, etc.).

It is probably also the component most subject to the desires of administrators to enhance the prestige of their schools or school districts by expanding their staffs. (For a hypothesis about administrative intensity based on the rationality of administrators, see Pondy, 1969.)

The preceding discussion suggests that it would be profitable for organizational research -- and particularly research on educational organizations -- to distinguish initially between fixed and variable personnel components. For fixed components it is necessary to question the assumptions which make them fixed and to consider possible alternative forms of organization which would alter the size of these components. For variable components it is important to examine in detail the organizational processes and features which affect their size, their structure, and their operation. For both types of factors it is essential to consider what the various categories of personnel actually do in order to study their structure and operation and to propose alternatives.

Problems of measurement

Organizational research and educational research on economies of scale have in common the fact that they use dependent variables which are percentages, or ratios, and that these ratios include the independent variable -- size -- in the denominator. This does not create a measurement problem unless the ratio is not considered to be the true variable; that is, if the theoretically interesting relationship is between size and the administrative ratio (or the per cent in administration, regardless of how administration is defined), then correct inferences about this relationship can be made by using these variables. It should be understood, however, that the magnitude and sign of the relationship may be strongly affected by the variances of the components (see Freeman and Kronenfeld, 1973). If, on the other hand, the theoretically interesting relationship is between size and number of administrators, then this relationship cannot be correctly inferred from the ratio or percentage variable (see Schuessler, 1973, for more detailed discussion). Freeman and Kronenfeld's (1973) discussion of problems of definitional dependency in the administrative intensity literature assumes that the component variables (size and number of administrators) are the theoretically meaningful or real ones; and that the ratios are introduced for purposes of standardization. The error they discuss is only an error under this assumption. The same remarks apply to expenditures per pupil as a dependent variable in its relationship to size (number of pupils). In educational research it is often the case that the ratio variables are intrinsically meaningful (if only by tradition). Expenditures per pupil

has been used extensively for making comparisons among school districts and for allocating funds. Teachers per pupil, which would be treated as a measure of the size of the "production" component in organizational research, is intrinsically meaningful as a measure of quality. The fact that teachers per pupil or its reciprocal is conventionally regarded as a measure of quality while at the same time it is a major component of expenditures per pupil implies either that expenditures per pupil is, by composition, a heterogeneous variable or that teachers per pupil is not a good measure of quality, or both. (This kind of ambiguity in the meaning of indicators is a serious problem in educational and organizational research; it deserves more attention than it has hitherto received.) Researchers interested in economies of scale have tried to control for quality by introducing independent variables to measure quality. A better method would be to decompose expenditures into those which unambiguously measure quality and those which unambiguously measure costs, and to analyze the components separately (see Duncan, 1966, and Schuessler, 1973, for discussions of decomposition. It may turn out to be impossible in practice to unambiguously decompose expenditures, in which case the whole issue of economies of scale should be reformulated.).

The converse of the ratio variables' being intrinsically meaningful is that the relationship between the components (e.g., number of administrators and number of pupils) is assumed to be spurious because of the size factor present in both. If the true relationship is assumed to be between these two components, then a correct way to test for economies of scale would be to see whether an increase in number of pupils (P) produces a

proportionate increase in number of administrators (A) by comparing a model which treats A as a linear function of P to a model which treats A as an exponential function of P, using an F-test to compare the variable explained by the different models (see Klatzky, 1970).

Ultimately the issue of whether to use ratio variables or their components is a substantive and theoretical issue, not a methodological one; however, researchers should be aware of the implications of treating either the ratio variables or their components as substantively meaningful.

The determinants and consequences of administrative intensity

Size. Size has traditionally been regarded as one of the major factors affecting the administrative component, and repeated cross-sectional studies have shown a negative relationship between size and the relative size of the administrative or staff component (see Freeman, 1973, for discussion of inconsistent findings from longitudinal studies). It is important to measure the effect of size adequately if only to control for it when examining the effects of other variables that may be spuriously related to the dependent variable through their common dependence on size. But we should be careful about the justification for studies of the effects of size; such studies are probably less frequently justified than is ordinarily believed. One major rationale for studying the relationship between size and the relative size of any personnel component has always been to determine whether there are significant economies of scale in larger organizations. This only makes sense, however, if organizations can alter their sizes in accord with the dictates of economies of scale. School districts have over time considerably altered their sizes through consolidation. There are

limits, however, to the amount of consolidation that can occur, given the way schools are structured -- limits set by transportation costs and/or the costs of boarding students away from home (assuming that boarding schools are politically feasible). In thinking about the possible effects of size, therefore, it must be recognized that the importance of this variable is seriously constrained by inelasticities in the size of school districts. On the other hand, Freeman and Hannan (1975) have recently shown that size has different implications for districts that are growing and those that are in decline; the leads they have developed seem well worth pursuing. It will also be of value to look at the effects of size reductions on the administrative ratio when such reductions result from deliberate decentralization.

All in all, however, since the relative size of personnel components is strongly constrained by structural, ecological, demographic, political, and social-psychological factors, these should receive much greater research emphasis relative to size than they have in the past.

Structural factors. School districts have traditionally been characterized by extreme decentralization and segmentation. Historically, public education began as a local function (see Cubberly, 1905), but the role of the state government has gradually increased in importance. This presents another problem. From the standpoint of local school districts, all people in the state Department of Public Instruction, for example, could be regarded as administrative personnel. There is no doubt that these personnel have increased in number over time, although this does not mean that they have increased relative to the size of the pupil population. The consequences of the growth of the state and federal bureaucracy in education deserve further study.

In spite of the consolidation movement, the basic structure of school districts in most states has not been altered. They are still relatively autonomous local units, and they are still segmentary -- that is, each of them is the structural and functional equivalent of every other. This means, among other things, that each district (and often each high school in multi-high school districts) must have the same minimum facilities and the same kinds of personnel; and the costs of providing these facilities and personnel limit the possibilities of economies of scale. One alternative to segmentary organization -- functional organization, wherein schools within districts or whole districts specialize in different types of programs, or where strictly educational programs are separated from social or recreational programs -- has hardly been tried. Whether functional organization has advantages (including economic, social, or political) or for whom it has advantages over segmentary organization is unknown. Obviously some kinds of functional organization will meet considerable resistance regardless of whatever economic advantages they might have. Specialization by type of program, for example, is likely to create invidious comparisons among students and to create a potential basis for rigid social class divisions. This is a strong reason for avoiding such forms of organization, and is a consequence which should be made explicit as part of any research. Not all kinds of functional organization are subject to the same criticism, however. There is no intrinsic reason why schools should perform all the functions they now do (many of which create invidious comparisons between students within schools -- e.g., social clubs, athletics programs. See Hollingshead, 1949, and Coleman, 1961, for descriptions of some of these invidious comparisons.).

Demographic and ecological factors. The geographical distribution of population and its age distribution obviously set constraints on economies of scale and on the ability to offer quality education. These have already been treated briefly in the discussion of size. The industrial composition of an area affects the tax base and the ability to finance educational programs. Industrial and socioeconomic composition also affect the demand for schooling, the kinds of programs offered, and the need for personnel to administer these programs. It would be useful to study in more detail how industrial composition affects demands for education; how people of differing social and occupational statuses actually do make demands on the school system for different kinds of programs; the consequences of these demands for the administrative structure; and the ability or willingness of the structure to respond to the demands of various client groups. In the educational research literature on economies of scale the demand for better quality in education is treated as a disturbance factor which must be controlled in order to accurately estimate the effects of size. Since it is recognized that demand for quality affects expenditures per pupil, it is worthwhile to investigate demand for quality in its own right.

Political factors. Political forces which affect the structure and the operations of school districts include many factors: (a) lobbying in state legislatures to maintain minimum state aid grants to wealthy school districts (see Coons et al., 1970), which increases the amount of money school districts have to spend and influences their decisions to hire more personnel of various kinds; (b) state policy on creation of a favorable industrial climate, which affects variation between states in the money available for education; (c) the composition of local school boards, which affects

their ability to consider alternatives to present structural arrangements and their desire to experiment with new programs; (d) the desire to maintain segregated schools, which has led in some areas to parallel structures for blacks and whites' -- an even more extreme form of segmentation than would occur otherwise; (e) the resistance of unions to reductions in the teaching force; (f) the ability of politically disadvantaged groups to make their needs felt in the educational bureaucracy, etc. The dynamics of political forces are particularly important to understand, either to propose alternatives which are both economically feasible and politically realistic; or to overcome resistance to what are politically unfeasible alternatives.

Social-psychological factors. Here we include factors such as competition among administrators to enhance the public image of their schools and school districts by paying higher salaries, hiring specialized personnel, etc. There are several kinds of research questions which can be asked: Does such competition actually exist? If so, for which categories of personnel does it make a difference? Are the personnel who are most visible to the outside world those which are most affected by competition among administrators? Do the internal social and political exigencies of educational administrators affect the ways educational services are provided to clients (cf. Blau, 1973)? How do clients' demands affect the administrative structure (see Clark, 1961; Zald and Denton, 1963)? How important is administrative discretion in determining the administrative structure? How much room is there for administrative discretion in such matters, given the constraints under which schools operate?

The effects of different forms of technology on the administrative structure, and the consequences of different technology for meeting the needs of clients should be studied; but since technology hardly varies in the existing educational system, existing technology must be compared on a cost-effectiveness basis to proposed alternatives.

Goals. An earlier section of this report deals with goal structures in detail. Surely, the relative size of personnel components will vary to some degree according to the objectives of those in control of policy. Therefore, the constancy of goals across school systems should not be taken for granted; rather, every effort should be made to admit variation in goals as an explanatory variable.

Alternative approaches to the study of administrative intensity

Research which compares administrative intensity in different types of organizations has limited utility for research in education, aside from its ability to suggest alternative ways of organizing educational services. The meaning of the "administrative," "staff," or "clerical" component in different kinds of organizations varies so markedly that it is dangerous to make simplifying operational definitions for the sake of comparability. On the other hand, comparisons between educational organizations (e.g., school districts) within a specified context suffer from the fact that the most important determinants of administrative structure are fixed rather than variable. One way out of this dilemma, as was mentioned earlier, is to foster research on administrative intensity and other structural properties that would compare existing structures to possible alternatives.

Such research would fall under the broad heading of "Administrative Experiments." They might be large scale or small, effected by experimental intervention or merely studied when they occur as the result of political or administrative decision. The methodological issues are covered in a later section of this report. We emphasize here that personnel and other structural variation will be only narrowly and conservatively understood if studied only within the confines of present administrative forms. Above all, we should not allow our desire to understand administrative intensity under traditional structural arrangements to impede our search for new and better ones. On the contrary, the devotion of resources specifically to the assessment of personnel structure in broader evaluations of administrative experiments is greatly to be encouraged.

V. ENVIRONMENT, STRUCTURE, AND STRATEGIES OF CONTROL

In previous sections of this prospectus, attention has been focused primarily on the relationship between organizational structure and a variety of organizational behaviors and outputs thought to be dependent upon structure, at least in part. From time to time, however, attention was turned to the possible causes of structural variation, on the twin premises that structure is indeed important in understanding education organizations and that we are therefore interested in understanding structure. It was the case for a half century or more that organizational structure was considered capable of being manipulated at will. One did not need to understand how it got that way and it could be changed radically in form in order to achieve certain desirable ends.

These assumptions underlay the normative thrust that accompanied a belief in the superiority of scientific management, bureaucratic organization, and supportive and participative supervision over the older structural forms they were proposed to replace. Organizational structure, however, is not necessarily so easy to manipulate.

Size, technology, and environment.

In the 1960's, scholars began to focus on structure as a dependent variable, that is, as a resultant of the state, or change in state, of specific forces other than the will of a chief executive. The size of the organization itself has in fact long been considered such a force (see Blau and Schoenherr, 1971), affecting at least some aspects of organizational structure. In addition, a few seminal works produced substantial interest in technology as such a causal force (Woodward, 1965; Perrow, 1967), and the environment of the organization as another (Burns and Stalker, 1961; Lawrence and Lorsch, 1967; Thompson, 1967). In this general theoretical perspective, it is possible and indeed natural for the structure of an organization to be molded by exogenous forces, and although it is possible for people to resist the pressure of these impersonal influences to some extent, the more they do so, the less successful the organization will be.

Organizational size has been covered in part in Section IV, on administrative intensity, and will receive further mention below. Since it is conceptually so simple and since it has been investigated so thoroughly as to leave us with relatively few large questions, we will not dwell upon it at length. The technology of an organization's production system varies richly across sectors, markets, and industries and much interesting research remains to be

conducted on the effects of such differences upon structural variation. In education, however, or in any other specific applied field, we will primarily be interested in comparing several organizations of the same type with one another (e.g., school districts), so that technology becomes relatively constant. It will be well worthwhile to augment existing cross-industry studies (see Hickson, et. al., 1969) of technology and structure, on the basis of recent theoretical developments to be reviewed presently, and to include education organizations among the industries surveyed. Much insight may be obtained by viewing the dominant or modal structure of education organizations in comparison with that of other types and noting, perhaps, the high probability that certain structural patterns are common in education because of the particular technology of this industry. Since this line of research would deal with education only in small part, however, it should not receive heavy emphasis in an education research program and we will not dwell heavily upon it here.

The environments of education organizations of a given type can differ markedly, however. Some school systems, for example, operate in metropolitan areas, some in rural areas, some in the inner city, others in suburbs, in stable communities, in boom towns, in university towns, in wealthy towns, in federally impacted areas, in heavily centralized state systems, in blue-collar communities, in politically competitive communities, and so forth. This variation in environment for organizations of one given type provides a fertile basis for research on the relationship between environment and structure and an opportunity, at the same time, to find substantial explanation for the differences in structure that appear in education. Research projects designed to contribute in a cumulative way to environment-structure theory in the field of education

are therefore greatly to be encouraged.

Structural typologies and strategies of control.

Before thinking in greater detail about how to use the concept "environment" in continuing organizational research, it is well to have firmly in mind just what it is that environment is proposed to explain. In a general way, we are trying to explain "structure." Historically, social scientists who were interested in a few particular aspects of organizational structure conceived hypotheses relating both technology and environment to those dimensions. Thinking about the concept of environment in recent social research has therefore been oriented toward using it in connection with those dependent variables. There is no reason why environment could not be considered as a cause of goal structure as well, although this relationship has not yet aroused active research interest. In the field of education, it makes a good deal of sense to explore this relationship (environment-goals) and to expect substantial rewards in terms of insight into the basic purposive orientations of schools and school systems. It might be, however, that the operational descriptions of environment most useful for this kind of research would be substantially different from those now being developed. We are at too early a stage in the conceptualization of both goal structure and environment to provide a great deal of guidance on such a question; it will perhaps be most efficient to refrain from trying to create totally new descriptive frameworks for environment until the present one is better understood, and (hopefully concurrently) until goal structures are better understood. Similarly, the concept of environment might be used to predict variation in effectiveness or efficiency directly,

without the mediation of structure. There is some work in progress on these relationships (Downs, 1975), but again it is too early to foresee the dimensions of the concept of environment that will be most productive in this newer theoretical context.

Most research effort on the environment-structure theme has emphasized dimensions of the authority, communications, and workflow structures. There is some tendency, in fact, to link such research with efforts seeking to determine whether structural dimensions cluster together so as to yield typical profiles, especially the Weberian "bureaucratic" type (e.g., see Child, 1972 and 1973). Perhaps the most frequently studied individual dimensions are centralization, specialization, and formalization. Centralization has been measured as the degree of delegation of authority, tallness of the official hierarchy, strictness of the hierarchical lines of authority and communication, participativeness in work groups, and autonomy of the focal organization from a parent organization. Specialization has been variously measured as the fineness of the division of labor, the number of distinct full-time jobs, the variety of the work, and the extent of professionalization. Formalization as a conceptual label has been applied to the extent to which specific rules and/or procedures exist, the extent to which such things are written down, the prevalence of printed forms, and even the availability of written orientation information to employees. Furthermore, it has been measured both by observation of the investigator and report of the employees with quite different results (for the various operationalizations referred to see Hall, 1963; Samuel and Mannheim, 1970; Pugh, et. al., 1968; Child, 1972; Reimann, 1973; and Pennings, 1974).

For two reasons, it will not be highly productive to investigate the relationship between environment and structure with the latter expressed in this sort of detail. One reason is that many of these operationalizations are not important enough to worry about as dependent variables. The other is that for many of the variables there is little theoretical or intuitive basis upon which to postulate a causal relationship with environment, except a most remote one.

The more likely direction for research, given the trends that are already visible in the literature, is (1) to discover how collections of structural variables such as the above cluster together in existing organizations, (2) to attempt to categorize the clusters as meaningful structural types, and (3) to attempt to explain the structural type that appears to characterize a given organization by reference to its environment, technology, and size. It begins to be clear that this direction is also unlikely to be productive and that therefore the trend to become more deeply involved in it should be resisted. Again, there are two reasons. The first is that these strange collections of variables often cluster only loosely and, when they do, apparently capriciously. Unnecessary confusion -- or diversion from theoretical development -- is produced by the finding that something called "formalization" clusters with something called "specialization" in one study, but not in another. The second reason is that, with a few exceptions to be discussed below, meaningful structural types are not emerging from such research, nor do they appear likely to emerge by continuing to use factor analysis and similar techniques on vaguely justified assortments of inputs.

A more reasonable approach is to specify the structural dimensions or types in which we have an interest, to find reasonable grounds, if they exist, for hypothesizing that these are explained in substantial measure by environment and similar forces, and then to investigate the connection empirically. One such typology, or typological dimension, is that running from "mechanistic" structure to "organic." There are already good grounds for suspecting that this dimension (roughly, a tight structure in which authority lies in a hierarchy as opposed to a more informal structure in which authority lies in expertise) is affected primarily by environment and technology (Burns and Stalker, 1961; Woodward, 1965; Perrow, 1967; for work in process see MacColl, 1975). There is some difficulty in that typologies are hard to work with as unitary dimensions; this one is no exception and satisfactory operationalizations have yet to emerge. Work along these lines could be quite useful, however, especially if the dimensionalizing of the structural concept were worked out within the context of and for application to a single applied area, such as education.

Another structural typology of great interest in this connection, only recently identified, is called the organization's "strategy of control" (Child, 1972). So far, the primary subdimensions of the typology seem to be centralization, specialization, and formalization, but in this new and specific perspective the conceptualization and measurement of these broad constructs are no longer so vague and permissive. The organization's strategy of control means the basic mechanisms it uses to guide the work that is done so as to be sure that it will result in the desired outputs. Two general kinds of mechanism

have aroused the greatest interest -- whether the guidance emanates primarily from a central source or from scattered sources, and whether it is exercised primarily through people or through impersonal sources. In this perspective, "centralization" comes to mean the extent to which decision-making authority is delegated rather than centrally retained, "specialization" means the extent to which the training and socialization of specialists are relied on to make the conduct of the work predictable, and "formalization" means the extent to which guidance is built into written procedures, machinery, or other impersonal sources.

There is no absolute necessity for combining these dimensions into a typology; it may in fact be more satisfactory to learn what impact environment has upon each of them separately. Furthermore, additional relevant dimensions will almost certainly be identified in the future. Insofar as it is of interest to look at them in combination as "strategies of control," however, one may recognize at least the possibility of commonly occurring types by noting joint location on these three underlying variables. The organization that is high on delegation and high on impersonal means of control (formalized procedures) is using what now tends to be called a bureaucratic strategy of control. High on delegation and specialization but low on formalized procedures indicates a functional-specialist strategy of control, much as in the concept of organic structure. Low on delegation but high on impersonal control indicates a mechanized or cybernetic strategy of guidance, including, perhaps, major applications of programmed instruction. Other recognizable combinations are of course possible, particularly since the component dimensions are continua rather than nominal categorizations.

To state a very general hypothesis relating environment to strategies of control, as we will do at the close of this section on environment and structure, it is convenient to quantify "strategies" conceptually so as to be applicable to all such combinations. We may therefore imagine an ultimately simple strategy in which all control is exercised personally by one individual at the head of an organization, and specify that the strategy becomes more elaborate as more decentralization, specialization, or formalization is introduced.

At this point in time, it appears as though the idea of strategies of control as a focal concept presents the most productive opportunities for research on the connection between environment and structure.

The consonance hypothesis.

Often, the hypothesis of a relationship between environment or technology and structure is accompanied by a second proposition, sometimes called the "consonance hypothesis" (Mohr, 1971). It states that certain organizational outcomes, particularly efficiency and effectiveness, are determined by the consonance of the organization's structure with its technology and environment, i.e., the more accurately the organization's structure is predicted by its environment and technology (the more it molds itself to these alleged pressures rather than resisting them), the more effective and efficient the organization will be. This hypothesis is quite different from one that posits structure as an intervening variate between environment-technology and effectiveness, for example, for in the latter case, should there be dissonance, certain structures are predicted to lead to certain levels of effectiveness regardless of the

environment and technology. The existing empirical evidence bearing upon a choice between the two propositions is scant and apparently inconsistent (Mohr, 1971); further insight would be of great importance and the gathering of additional evidence is therefore to be encouraged in environment-structure research in education.

The environment.

Finally, having ascertained that an interest in variables such as effectiveness and strategies of control presents a reason for studying the environment of an organization, it is necessary to ask just what is meant by the environment and how it might be measured. The state of the art in this regard is not advanced.

There are two major challenges. The first is to develop widely acceptable criteria for separating the environment that is relevant to an organization from the part that is irrelevant. The environment of any object is of course infinite, but we only care about certain segments of it, or certain entities within it. Which parts shall we look at or refer to when we attempt to quantify "the environment?" By and large, the question has not been met squarely in the small amount of research in this area conducted to date (Burns and Stalker, 1961; Emery and Trist, 1965; Lawrence and Lorsch, 1967; Thompson, 1967; Duncan, 1972). There are hints and tendencies, however, indicating that the following criteria will be most useful: the relevant environment is separated from the irrelevant in terms of a threshold of (a) dependence of the organization upon something outside of itself or (b) impact of outside forces upon the structure and activities of the organization. To apply the criteria,

it will no doubt be necessary to ask questions or make observations within the focal organization. There is room for a great deal of research and ingenuity in the development of systematic means of operationalizing the relevant environment in each case by questions and observations in the focal organization.

The second challenge is that of dimensionalizing the environment. In this case, a fair amount of progress has been made and is presented in the group of works cited just above. One might summarize much of it by specifying six measurable characteristics of the environment of any organization, which fall fairly readily into two categories. There is first of all the size or scope of the relevant environment of an organization -- the number of different organizations or other entities or forces that are "out there" in some important way, or the number of ways in which the organization is dependent upon or influenced by such entities. Second, there is the heterogeneity of these entities. Third, there is the level (above the threshold) at which the organization is dependent or is influenced. These three characteristics indicate the magnitude of the relevant environment. Fourth, there is the stability of the entities identified. Fifth, we have the interdependence of these entities among themselves -- a dimension of complexity introduced by the need for the focal organization to deal with relationships in the environment, as well as separate entities. And last, we are concerned with the availability of information about the entities identified. These latter three characteristics indicate the uncertainty of the environment. It is thus possible to test hypotheses involving any of these six dimensions on its own, or to develop theoretical propositions in more general terms with reference to the magnitude and

uncertainty of the relevant environments of organizations. This is not to say that closure has been reached in dimensionalizing the environment; on the contrary, the art is again in a rather tentative state and elaboration and refinement are expected to arise from further empirical research.

One final problem must inevitably be faced: the decision whether to measure the environment by direct observation or as it is perceived from within the organization. Both have been done. It is important to recognize that both may need to be done even within individual studies. Perceptual measurement is apparently called for in testing the primary environment-structure hypothesis; there is a teleology involved that leads us to expect people within the organization to respond to external demands as they perceive them. Objective measurement, however, would be needed to test the consonance hypothesis; we expect effectiveness to result from the consonance of structure with the environment as it actually is, not as organizational members (perhaps mistakenly) perceive it (cf. Downs, 1975).

In this section, then, we have specified what is meant by "environment" and by "structure" in terms of a proposed relationship between them. The fundamental hypotheses to be tested are (1) that the magnitude and uncertainty of the environment, as well as the size of the organization and its technology, determine structure, particularly the elaborateness of the strategy of control, and (2) that the consonance between environment-size-technology and strategy of control determines effectiveness and other outcomes. Using individual subdimensions of these concepts, a large number of more specific hypotheses are available, and it is in these that the greatest interest will initially lie. The implications of the findings for understanding education organizations are important, and research into these questions is strongly recommended.

VI. INTERORGANIZATIONAL ANALYSIS

Ordinary organizational analysis as applied to education would entail, for the most part, the explanation of some characteristics of the focal organization in terms of others. Rarely would the research strategy direct the investigator to go outside of that organization for specific observations to be used in analysis. There are a few exceptions, a notable one being the recent trend of studies on "environment and organization," which is covered in the previous section. It is broadly recognized, however, that an education organization, especially the local school district, is ordinarily involved in a fairly specific multi-organizational network, and that what happens in the focal organization depends in immediate and important ways upon conditions and events in the other units of the network. The network for a district, for example, might include a union, one or more professional groups, local citizen groups, state education agencies, similar or neighboring districts, and city and county legislatures and governmental agencies. Therefore, the established branch of organizational studies that is commonly labeled "interorganizational analysis" would seem to have compelling relevance to the applied field of education.

The relevance is unquestionable, but there are unfortunately few results from this tradition that can be applied directly, nor does there appear to be an identifiable line of inquiry currently pursued that promises theoretical and practical payoff in the future. It might be said in a sense that the research subfield has not yet settled down. Several researchers long ago conducted studies -- empirical or theoretical -- of multi-organization sets and labeled the research "interorganizational analysis." The semblance

of a disciplinary subfield was thus created, and subsequent researchers have placed themselves in it nominally where they, too, addressed themselves to interacting groups of organizations. In truth, however, the research has not been cumulative; very few treatments of the topic are closely related to others. The area is diffuse; almost anything that bears on groups of organizations has been fair game for inclusion. There are, however, too many organizations that may be interrelated and too many ways in which they relate (e.g., see Guetzkow, 1966) to expect order to arise without self-conscious efforts to build theory cumulatively. The next steps in this unquestionably important research area should therefore be dominated by two concerns: (1) the need to be more clear and specific about the questions we want to answer with such research, and (2) the need to move toward the operationalization of a multi-organization unit, so that the questions can be answered systematically.

Considering the first of these two concerns, we are concerned with the need to bring much more focus to the area by deciding which of the hundreds of possible questions about multi-organization groups it is most desirable to attack, at least initially. What do we really want to know? This question is commonly asked about two distinct units of analysis; confusion arises from not keeping them distinct. One is an organization that is part of a multi-organization group, a "focal" organization (Evan, 1966); the other is that group as a whole (cf. Van de Ven 1974, 116-119). Two streams of research are suggested that are no doubt mutually relevant but are for the most part disjoint.

Research on the focal organization.

In one stream of research, then, the dependent variables -- the phenomena one seeks to understand -- are characteristics of a single organization. The research enterprise is distinguished from ordinary organizational research in that the attempt to understand them is made through the organization's "membership" in a larger network. Even so, research on environment and organization would seem to include this approach, since the other organizations in a group surely form part of the environment of the focal organization. We carve out a separate niche, however -- a subset that merits standing in its own right -- when we use as explanatory forces properties of the specific, identified multi-organization group rather than either the environment as a whole (e.g., "the uncertainty of the environment") or a highly specific organizational segment of it (e.g., "competitors"). This stream of interorganizational analysis or research, then, places the focal organization in a specific multi-organizational group and seeks to explain characteristics of the focal organization either by characteristics of the group itself or of the focal organization's relationship to that group.

The questions we would seek to answer, with at least one notable exception, are the same as those in much intra-organizational research. We will be interested in the contributions of interorganizational relations to organizational structure and strategies of control, to goal structure, to effectiveness and perhaps even more to efficiency, to morale and commitment, to the levels of conflict and cooperation within the focal organization, and so forth.

The important addition lies in dimensions that are themselves relational, particularly the autonomy of the focal organization. It is recognized that although a given organization may not be subordinated in any formal way to a central authority, still, it may be systematically constrained in its own behavior more or less severely by its membership and role in the multi-organizational group. Writers in interorganizational analysis have displayed a prominent interest in the lack of autonomy of formally autonomous organizations (see, for example, Evans 1966, 180-184). On the face of it, it does seem intriguing to note that some organizations are less autonomous than others because of their position in a multi-organization set, and it therefore appears worthwhile to investigate just what sort of position vis-a-vis the other organizations in the set make for greater or less autonomy. Research attention must also be directed, however, to the implications of greater or less autonomy. It is not enough simply to be intrigued; it is important to know wherein the education organization with less autonomy functions differently than the one with more (see Hasenfeld, 1972, for some highly specific results). Is there a difference in efficiency? Is the ratio of administrative personnel to production personnel any greater? Does personnel quality differ? Morale? Decision-making style? Centralization of intra-organizational decisions? The relation of autonomy to these other factors might be considered an issue in intra-organizational research, but the results are clearly important for the justification of autonomy as a dependent variable in interorganizational analysis. As a final note, it should be pointed out that publications in

interorganizational analysis have little to say about the operationalization of "autonomy," and what little there is tends to define the variable by its interorganizational causes, leaving no room to explain it by the same factors. Instead, there is at least one operational model from intra-organizational work that should be consulted as a guide (see Pugh et. al., 1968).

Given the dependent variables for this stream of research -- the characteristics of focal organizations that we seek to understand -- it remains to consider the independent variables, i.e., the interorganizational explanations. As noted, these are properties of the multi-organization network itself or of the relations of the focal organization to the remainder of the network. Several examples of explanatory forces have been noted by writers in the field, including dependence upon the other organizations for resources (Levine and White, 1961, 589-592) or for receipt of output (Hasenfeld, 1972), accessibility of information about the other organizations (Hasenfeld, 1972), the size of the network (Evan, 1966, 183), the similarity or complementarity of goals (Evan, 1966, 182; Van de Ven, 1974, 119), and others. The great preponderance of these suggestions have grown out of "armchair" work rather than empirical study. Therefore, it is reasonable to expect that the field of productive variables will expand or otherwise change as investigators seek systematic explanations for organizational phenomena through empirical interorganizational research.

Research on the multi-organizational group.

The second stream of research in this area would focus on the group itself, with no identification of any particular organization as focal. In a sense, it is similar to intra-organizational research, since both causes and effects

are sought within a single unit (in this case the interorganizational net); it is basically a closed-system approach. It is different from single-organization research, however, primarily because the unit studied in this case represents related parts of a whole that are not bound together by a common, formal authority structure. For this reason, the questions that we seek to answer are in large measure different from those addressed in intra-organizational research. Many of the dependent variables will sound familiar, some will appear slightly unusual for organizational research. We will be interested, for example, in understanding levels of cooperation and conflict in the net as a whole, characteristic modes of decision making among organizations (persuasion, bargaining, etc. -- cf. Warren, 1967), rates of interactions of various types, stability of the net, dominant modes of coordination of activities, and other characteristics. However, explanations of variation in these characteristics are likely to be different than they are in ordinary organization theory, although a notable lack of attention to such questions makes it difficult to suggest just what the explanations might be. To a great extent, the independent variables with reasonable explanatory power may be the same as in studies of focal organizations, reviewed above. The most important step to be taken for the formulation of variables, however, both independent and dependent, is probably to develop concrete ways of characterizing the structure of interorganizational nets. This will be discussed below. It seems quite apparent that in large measure the structure of various types of relations among the organizations of the group is both what we want to explain and how we want to explain it (i.e., some structural relations are explained by others).

Little has been done empirically in this area to date, even though much interest in it is expressed in the literature. This is no doubt due in part to the practical difficulties inherent in systematic research on groups of organizations. The embeddedness of education organizations in interorganizational groups appears to be so important for understanding the educational function, however, that the practical problems of research should be overcome. Researchers have surmounted the difficulties of comparative research across large numbers of single organizations. The extension to numbers of groups is not trivial, but treated as an extension it should be workable. In addition, there is much to be learned from case studies within one or two interorganizational nets, and from simulation designs that answer questions about the development of networks of relations over time.

Interorganizational structure.

What is an interorganizational net, or set, or group? How can we recognize one when we see it? How do we know when we have come to the end of it? In what terms can we describe its structure? These questions, which would appear to be basic to interorganizational analysis, have not yet been addressed in a rigorous way. Two kinds of contribution are needed: research to determine the relations that are best used to define interorganizational nets, and research that in some sense provides structural models.

Taking the latter issue first, there already exists an inclination to characterize sets of organizations as points connected by lines (Evan, 1966;

Van de Ven, 1974). It would appear that digraph theory (Harary, Norman, and Cartwright, 1965), which is an elaborate structural mathematics based on points connected by lines and which was developed largely with social science utilization in mind, would be a tool of great power and helpfulness in interorganizational analysis. It would, in brief, provide three types of benefits: a large, precise vocabulary for describing all aspects of structure; techniques and formulas for calculating many important quantitative aspects of structure (e.g., the average "distance" between organizations); and theorems that enable the deduction of many structural properties from knowledge of others. It should be possible by means of such a structural model for the investigator to perceive important properties of an inter-organizational net that are not perceived even by those who work within it.

It is possible to take any group of organizations in which one is interested, characterize its structure by points and lines, and learn something about it, perhaps a great deal, from graph theory. A great many of the variables needed in interorganizational research could be generated in this manner. A second possibility is to begin with structures about which a great deal is known in graph theory (e.g., a "block") and define interorganizational nets so that they conform to such structures. The two approaches are complementary, at least in the exploratory phases of the application of graph theory to interorganizational research. The second has the particular advantage of circumscribing the idea of interorganizational groups, so that a stopping point can be reached in decisions about which organizations to include.

In sum, the detailed ways in which graph theory can be most fruitfully applied to interorganizational analysis remain to be worked out in studies especially devoted to the purpose, but there is little question about the need in this field to incorporate specific structural models of some sort. Since structure is clearly so important a factor in understanding groups of related organizations, progress will be greatly enhanced by rendering structure in clear and concrete rather than zero, vague, or ambiguous terms.

Interorganizational relations.

The research suggested in the foregoing section would help us to understand how to express the structure of an interorganizational group. If it is to be done with points and lines, as seems eminently reasonable, that research would also help us to decide how many points to include in a given instance. An essential factor would still be missing, though: How do we know when to draw a line between the points that represent two organizations? What sort of a relation must exist between two organizations in order for us to say that they are "related?"

Many responses are possible. An arrow from one point to another might be included, for example, when to a prescribed extent, one organization depends on another one for something -- resources, legitimacy, consumption of output, information, cooperation, or any combination of these. The arrow might be drawn instead when one organization has influence over another, or it might be drawn on the basis of a level of interaction between the two, or on the basis of complementarity of functions. Unfortunately, all of these criteria for declaring organizations to be directly related seem important,

and all have been mentioned in existing writings as being important, but to use them all at once might be to confuse an analysis hopelessly.

To date, there have been no research efforts designed to find the ways of defining "related" that will contribute most to the specification of interorganizational networks. The task is not a minor one, even for just one applied field such as education, because the most productive criteria might be different for different research purposes. For example, dependence of some sort might be the most useful criterion when a focal organization is the object of study, but interaction-influence rates might take precedence when the properties of whole nets are to be understood. Again, it is desirable to specify the structure of an interorganizational group in order to understand it (or its parts), and it is essential to define the connectedness between organizations in order to specify the structure.

In sum, the area of interorganizational analysis is important but not yet highly developed. Research is needed on how to operationalize the idea of an interorganizational group. From there, specific designs need to be consciously related to specific questions, regarding either the properties of the group itself or the properties of a focal organization that may be illuminated by reference to the group.

VII. THE METHODOLOGY OF ADMINISTRATIVE EXPERIMENTS

Throughout the foregoing pages, numerous suggestions for the conduct of administrative experiments are implicit, if not explicit. In order to find out if another way is better, the best strategy and often the only reasonable strategy may be to try it. Any such trial, however casual,

is an administrative experiment, but its value may be enhanced by orders of magnitude if it is observed with care and system. It is not the purpose of this section to suggest that particular administrative experiments be conducted, but rather to suggest research, often modest research, that will increase the potential utility of all such experimentation in the field of education.

Education, and research in education, has long been identified, along with agriculture, with the introduction of major advances in the methodology of field research (Gage, 1963). The importance of combining the theoretical and methodological contributions of the academic researcher with the resources and opportunities under the control of the administrator is central to the concept of administrative experimentation advanced by Campbell and others (Campbell, 1969; Thompson, 1974). In contrast to field experiments, where the administrator may have no separate interest in the results, or action research, where the organization's purposes may require sacrifice of experimental control, the strategy of administrative experimentation is to work out, cooperatively, interventions and experimental designs which serve the purposes of both the experimenter and the administrator. In the ideal case, the administrator is also the experimenter and carries out the experiment not only to obtain some organizationally desirable result but also to obtain confidence in the relation between the intervention and the result obtained. The sense of an administrative experiment is preserved, however, wherever the two purposes are combined.

The present size, visibility, and nature of the services provided by educational organizations impose a critical impetus to improvement of

all parts of the process. These same factors make it both difficult to bring about and to evaluate improvements. The "normal" process of theory development and testing in the laboratory and then extension to field situations is no longer adequate; and the alternative of trial by error and muddling through, is becoming less acceptable in view of the cost of programmatic changes and the increasing demand for credible evaluations. The improvement of theoretical and methodological tools, and their application systematically to field situations, have therefore become essential.

There are a number of approaches for methodological research which are suggested, including the following: surveys of past and current practice in field experimentation, and evaluation of the theories and methods used, both successfully and unsuccessfully; development of new theoretical and methodological tools; post hoc analyses (or reanalyses) of individual experiments; and preparation of aids or guides for use by experimenters.

All of these approaches are comprehended in the discussion that follows.

The scope of the subject matter may perhaps best be indicated and treated by referencing key phases in the process of experimentation -- initiation and planning, selection of design, problems of complexity and scale, measurement, and implementation.

Initiation and planning

While much informal experimentation is at the level of "let's try this and see if it works," the anticipated investment of research resources and corollary administrative risks presents, in many cases, major difficulties in the initial choice of experiment, and the further defining required in planning. Studies of successful and unsuccessful

experiments, particularly in educational organizations, may, when coupled with analytical and theoretical developments, provide guidance to administrators and experimenters (see Havelock, 1969).

There are some obvious benchmarks: the intervention (independent variable) should be one that can be manipulated experimentally, and, if the experiment is a success, could be manipulated by others desiring similar results; the expected or desired result (dependent variable) should be measurable, and should be of some (potential) use to the organization or others; the relationship between these variables should be (or is expected to be) reasonably direct, and alternative hypotheses should be reasonably controllable.

The choice of the specific experiment, or the evaluation of a proposed experiment, even at this primitive level may require some sophistication not only with respect to the organization within which the experiment is to be conducted but also with respect to relevant theory and experimental design (see Riecken and Boruch, 1974). The following questions are now being researched or need to be researched, all of them represented in the literature on field experimentation: How does one determine the political and financial feasibility of the proposed experiment? How does the process for authorizing interventions vary with the characteristics of the intervention or the organization? What are the goal-defining processes for assessing dependent variables? What are the advantages and disadvantages of the several possible relationships between the administrator and the experimenter (on his staff, or an independent outsider)?

Selection of design

The major forms of experimental (and quasi-experimental) design are set out in the literature (see Campbell and Stanley, 1966; Suchman, 1967). It is not always clear what choices are available for a particular experiment, however, and, where a choice among designs is available, it is not always clear how tradeoffs should be analyzed, to what extent detail planning is required to evaluate feasibility, how sample size and duration of the experiment should be determined, or how complete the planning should be before the introduction of the intervention. Post hoc evaluations suggest that errors in design are, in many cases, anticipatable, and it seems clear that some experiments which have provided uninterpretable results could, and should, have been so identified based upon a more thorough initial analysis. Quite valuable guidance on these issues is already available in methodology texts and some extensive reports or reviews of field experiments, although the form of exposition and scattered state of the information may make it difficult to use effectively. The practice of research does not reflect these teachings. Some additional theoretical development and testing will be required, but much can also be done by bringing what is known together and putting it in comparable terms and usable form. The costs and disappointments of past research would seem to warrant the direction of resources to this area.

Problems of complexity and scale

The size or scale of experiments ranges from very large, programmatic interventions, to modest "one-shot case studies," where intervention and "effect" may be reasonably expected to be localized (see Rossi and Williams,

1972). The theoretical distinctions, sometimes described as "level of analysis," have been subjects of concern in many disciplines. In some cases, explicit frameworks or transforms exist to relate both macro and micro levels; in others, the emphasis is on developing internally consistent frameworks at the several levels (see Glaser and Strauss, 1967). There are, of course, theorists concerned directly with relational theories, including the most general (see Neurath, 1955).

Large scale, programmatic experiments, such as those involved in major political reorganizations, e.g., decentralization or consolidation, present basic methodological problems for which explicit theories provide little guidance (cf. Berlin, 1974). Is it possible, is it practical, is it justifiable to consider or to expect a reasonably credible evaluation, in experimental terms, of such large-scale experiments? Are there methods of aggregation, are there conceptual models, are there analytical models which provide a framework within which one can obtain, and assess, results? For what set of variables, with what range of coefficients, can what levels of confidence be expected? Are there characteristics of variables, of sites, of time domains, which provide clues that will allow confident predictions or expectations? Education is an area of urgent need for findings on this general question, and research in the systems area, i.e., analysis, engineering, reliability and quality control, provides a potential theoretical basis for exploration.

At the other end of the scale, the "one-shot case study," is largely neglected in the formal literature, but may be the major experimental form in practice. Are there process descriptive theories which may account

for this? Can we develop theories to discriminate between scientifically acceptable (credible) methods and those which provide little confidence?

As a practical contribution to the administrator, this might well be one of the most critical areas for the development of theory and method. The literature here is scattered widely from the philosophical to the pragmatic and is usually in the form of implicit assumptions or unstated inconsistencies, often central to major differences in approach, such as in probability theory. It is possible, however, to review the literature critically and, perhaps with the aid of additional studies, to reach conclusions of substantial value to administrative experimentation.

Underlying much of the above is the generic problem of decomposition. One of the fundamental characteristics of the strategy of science is the identification and isolation of a particular problem (the relationship of one particular variable to another); in the laboratory, and in abstract form, this is often realizable. In real-world experimentation, the complexity of the situation often makes such isolation or decomposition extremely difficult. There are now powerful theories advanced in areas of mathematics, however, which provide potential applications and extrapolations that will contribute significantly to the design and analysis of real-world experiments (see Himmelblau, 1973).

Measurement

Measurements of dependent variables present one important and challenging area for research (cf. Sjogren, 1971). Successful and unsuccessful experiences with identifying and measuring dependent variables in educational organizations should provide an extensive base for administrative experimenters.

In addition to measurement techniques, it should be possible to evaluate the variety of techniques for using indicators and other surrogates for variables not directly measurable. In organizations which provide service rather than products, measures of creativity and productivity, or reasonable surrogates, are of obvious priority (see Hatry and Fisk, 1975; Ross and Burkhead, 1974).

Further, the credibility of experimental results may be largely, if not solely, measured in terms of those plausible rival hypotheses for which adequate control has not been achieved (see Campbell and Stanley, 1966). Control may be achieved by randomization, by measurement and factoring out, or by "fixing" the value of the extraneous variable (control in the physical laboratory sense). For experiments which involve novel or exploratory situations, an organized and systematic base of prior theory and method may not be available. Is it possible to analyze winning experimental strategies and discover systematic approaches to the identification of alternative hypotheses? Are there significantly different levels of "acceptable" control for certain classes of rival hypotheses? Are there types of parameters for which there are established, preferred methods of control? These questions are researchable. In some cases, for example, theoretical contributions in utility theory, and engineering concepts such as sensitivity analysis and the evaluation of tradeoffs, may provide a basis for research. The problems are important in that they systematically limit our ability to carry out feasible and credible experiments.

In on-going organizations, data on potential variables is characteristically collected for other purposes; interventions and control

variables are often represented by policies and procedures; many dependent variables are regularly subject to measurement and recording for organizational purposes. These institutional records provide an important base, particularly for experiments employing time series, and provision for their use in subsequent experiments may introduce little or no additional cost (see Campbell, 1967). Research might provide an insight into those forms of record keeping in education which are most useful for research purposes, including, for example, issues of availability for machine data processing, assurance of quality and completeness, and period of retention. A more difficult, but potentially valuable, area of research would be the identification of those key variables which are common to a wide variety of experiments in education, and for which modest changes in record keeping would assure that the records are available when needed (see Bauer, 1969).

Implementation

Another key programmatic area is that of implementation (see Schultz and Slevin, 1975). Some experimental programs are proposed, and justified, on the basis that successful results will be "used" by other organizations, or will be continued. The record of implementation of even "successful" programs suggests that research to understand why implementation occurs in some cases and not in others is needed. Is failure to implement arbitrary? Is it based on the often low credibility of the results of the experiment? Is it because the results are not desired? Fairly, the present state of the art with regard to providing answers to these questions is pre-scientific. Recognizing the overriding importance of implementation, however, several Federal agencies have now demonstrated a keen interest in developing

answers systematically. The value of experimentation to education will be greatly enhanced through the support of research on implementation in education itself and other applied fields of public service.

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